

much larger number of species than was formerly suspected, and that the modification is invariably in the direction of protection. The experiments prove also that the stimulus prompting the colour change is given by the colour of the surroundings, but the precise means by which the stimulus is conveyed to the pigment-secreting cells has not yet been made out. This part of the work is no doubt the most difficult to deal with from the experimental side, but any objection to the Darwinian explanation which may be urged from the point of view of our ignorance of the nature of this correlation between an external stimulus and the power of secreting a particular colour applies with equal or greater force to the theory of "direct action" upon which so much stress is laid by the new Lamarckian school. The difficulty in the way of completing the explanation of this kind of action is of precisely the same nature as that which meets us when we attempt to explain the power of colour adaptability in a frog or fish as depending upon a colour stimulus, which in these cases is known to be conveyed through the eye. All that is contended for is that the power of adaptation has been conferred by natural selection, an agency capable of dealing with complex physiological relationships in precisely the same way that it deals with all other kinds of variations. In these cases of variable protective colouring we are concerned with the *origin* of the initial variations only in the same manner that we are concerned with their origin in ordinary cases of protective resemblance. Why the colour variability should always be restricted to the limits of protective shades is perfectly intelligible from the purely Darwinian stand-point, but is, as it appears to me, absolutely devoid of meaning if we accept the theory of "direct action."

R. MELDOLA.

August 18.

MODERN VIEWS OF ELECTRICITY.¹

PART IV.—RADIATION.

IX.

SO far as we have been able to understand and explain electrical phenomena, it has been by assuming the existence of a medium endowed with certain mechanical or *quasi*-mechanical properties, such as mobility, incompressibility or infinite elasticity of volume, combined with a certain amount of plasticity or finite elasticity of shape. We also imagined the medium as composed of two opposite constituents, which we called positive and negative electricity respectively, and which were connected in such a way that whatever one did the other tended to do the precise opposite. Further, we were led to endow each of these constituents with a certain amount of inertia, and we recognized something of the nature of friction between each constituent and ordinary matter.

Broadly speaking we may say—

(1) That *friction* makes itself conspicuous in the discussion of current-electricity or the properties of conductors, and that the laws of it are summarized in the statement known by the name of Ohm, viz. that the current through a given conductor is proportional to the force that drives it, or that the opposition force exerted by a conductor upon a current is simply proportional to the strength of that current.

(2) That *elasticity* is recognized as necessary when studying the facts of electrostatics or the properties of insulators—electric displacement and recoil, or charge and discharge: the laws having been studied by Faraday, and the relative pliability (or shearability if there were such a word) of the medium in different substances being measured and stated in terms of that of air as their specific inductive capacity, K .

(3) That *inertia* is brought into prominence by the facts of magnetism, studied chiefly perhaps by Thomson, who has called the relative density of the medium in different substances their magnetic permeability or magnetic inductive capacity; the ratio of its value for any substance to its value for common air being called μ .

(4) That the *doubleness of constitution* of the medium

—its being composed of two precisely opposite entities—is suggested by the facts of electrolysis, by the absence of mechanical momentum in currents and magnets, and by the difficulty of otherwise conceiving a medium endowed with rigidity which yet is perfectly fluid to masses of matter moving through it.

With the hypothesis of doubleness of constitution this difficulty disappears. The ether as a whole may be perfectly fluid and allow bodies to pass through it without resistance, while its two components may be elastically attached together and may resist any forces tending to separate them with any required rigidity. It is like the difference between passing one's hand through water, and chemically decomposing it; it is like the difference between waving a piece of canvas about, and tearing it into its constituent threads.

To put the matter boldly and baldly: we are familiar with the conceptions of matter and of ether, and it is known that the two things react on each other in some way, so that although matter appears to move freely through a free portion of the ether, yet another portion appears to move with matter as if bound to it. This mode of regarding the facts is as old as Fresnel. We now proceed a step further, and analyze the ether into two constituents—two equal opposite constituents—each endowed with inertia, and each connected to the other by elastic ties: ties which the presence of gross matter in general weakens and in some cases dissolves. The two constituents are called positive and negative electricity respectively, and of these two electricities we imagine the ether to be composed. The tie between them is dissolved in metals, it is relaxed or made less rigid in ordinary insulators. The specific inductive capacity of a substance means the reciprocal of the rigidity of its doubly constituted ether.

Let us call this rigidity k , so that $k = \frac{1}{K}$.

The neighbourhood of gross matter seems also to render ether more *dense*. It is difficult to suppose that it can really condense an incompressible fluid, but it may load it or otherwise modify it so as to produce the effect of increased density. In iron this density reaches its highest known value, and in all substances the density or inertia per unit volume of their ether may be denoted by μ , and called their magnetic permeability.

Let it be understood what we are doing. In Part I. we discussed effects very analogous to those which would be produced by an elastic incompressible medium (roughly like india-rubber or jelly). In Parts II. and III. we discussed effects suggesting, and more or less necessitating, the idea of a property of the medium very analogous to inertia; and we were also led to postulate a doubleness of constitution for the medium, so that shearing strains may go on in it and yet it be perfectly fluid as a whole. We are now pushing these analogies and ideas into greater definiteness and baldness of statement. We already know of a continuous incompressible fluid filling all space, and we call it the ether. Let us suppose that it is composed of, and by electromotive force analyzable into, two constituents; let these constituents cling together with a certain tenacity, so that the medium shall have an electromotive elasticity, though mechanically quite fluid; and let each constituent possess inertia, or something so like inertia as to produce similar effects. Making this hypothesis, electrical effects are to a certain extent explained. Not ultimately indeed—few things can be explained ultimately—not even as ultimately as could be wished; for the nature of the connection between the two constituents of the ether and between the ether and gross matter—the nature of the force, that is, and the nature of the inertia—remains untouched. This is a limitation to be clearly admitted; but if that were the only one—if all else in the hypothesis were true—we should do well, and a distinct step would have been gained. It is hardly to be hoped that this is so—hardly to be expected that the bald statement

¹ Continued from vol. xxxvii. p. 368.

above is more than a kind of parody of the truth ; nevertheless, supposing it only a parody, supposing what we call electromotive elasticity and inertia are things capable of clearer conception and more adequate statement, yet, inasmuch as they correspond to and represent a real analogy, and inasmuch as we find that a medium so constructed would behave in a very electrical manner, and might in conjunction with matter be capable of giving rise to all known electrical phenomena, we are bound to follow out the conception into other regions, and see whether any other abstruse phenomena, not commonly recognized as electrical, will not also fall into the dominion of this hypothetical substance and be equally explained by it. This is what we shall now proceed to do.

Before beginning, however, let me just say what I mean by "electromotive elasticity." It might be called chemical elasticity, or molecular elasticity. There is a well-known distinction between electromotive force and ordinary matter-moving force. The one acts upon electricity, straining or moving or, in general, "displacing" it ; the other acts upon matter, displacing it. The nature of neither force can be considered known, but crudely we may say that as electricity is to matter so is electromotive force to common mechanical force : so also is electromotive elasticity to the common shape-elasticity or rigidity of ordinary matter : so perhaps, once more, may electrical inertia be to ordinary inertia.

Inertia is defined as the ratio of force to acceleration ; similarly electric inertia is the ratio of electromotive force to the acceleration of electric displacement. It is quite possible that electric inertia and ordinary inertia are the same thing, just as electric energy is the same with mechanical energy. If this were known to be so, it would be a step upward towards a mechanical explanation ; but it is by no means necessarily or certainly so ; and, whether it be so or not, the analogy undoubtedly holds, and may be fruitfully pursued.

And as to "electromotive elasticity," one may say that pure water or gas is electromotively elastic, though mechanically limpid ; each resists electric forces up to a certain limit of tenacity, beyond which it is broken ; and it recoils when they are withdrawn. Glass acts in the same way, but that happens to be mechanically elastic too. Its mechanical elasticity and tenacity have, however, nothing to do with its electric elasticity and tenacity.

One perceives in a general way why fluids can be electrically, or chemically, or molecularly elastic : it is because their molecules are doubly or multiply composed, and the constituent atoms cling together, while the several molecules are free of one another. Mechanical forces deal with the molecule as a whole, and to them the substance is fluid ; electrical or chemical forces deal with the constituents of the molecule, setting up between them a shearing strain and endeavouring to tear them asunder. To such forces, therefore, the fluid is elastic and tenacious up to a certain limit. Extend this view of things to the constitution of the ether, and one has at least a definite position whence to further proceed.

It may be convenient and not impertinent here to say that a student might find it a help to re-read Parts I. and II. in the light of what has just been said : remembering that, for the sake of simplicity, only the simple fact of an elastic medium was at first contemplated and insisted on ; no attempt being made to devise a mechanism for its elasticity by considering it as composed of two constituents. Hence the manifest artificiality of such figures as Fig. 6 (NATURE, vol. xxxvi. p. 559), where fixed beams are introduced to serve as the support of the elastic connections. But it is pretty obvious now, and it has been said in Part III., that a closer analogy will be obtained by considering two sets of beads arranged in alternate parallel rows connected by elastic threads, and displaced simultaneously in opposite directions.

Recovery of the Medium from Strain.

We have now to consider the behaviour of a medium endowed with an elastic rigidity, k , and a density, μ , subject to displacements or strains. One obvious fact is that when the distorting force is removed the medium will spring back to its old position, overshoot it on the other side, spring back again, and thus continue oscillating till the original energy is rubbed away by viscosity or internal friction. If the viscosity is very considerable, it will not be able so to oscillate ; it will then merely slide back in a dead-beat manner towards its unstrained state, taking a theoretically infinite time to get completely back, but practically restoring itself to something very near its original state in what may be quite a short time. The recovery may in fact be either a brisk recoil or a leak of any degree of slowness, according to the amount of viscosity as compared with the inertia and elasticity.

The matter is one of simple mechanics. It is a case of simple harmonic motion modified by a friction proportional to the speed. The electrical case is simpler than any mechanical one, for two reasons : first, because so long as capacity is constant (and no variation has yet been discovered) Hooke's law will be accurately obeyed—restoring force will be accurately proportional to displacement ; secondly, because for all conductors which obey Ohm's law (and no true conductor is known to disobey it) the friction force is accurately proportional to the first power of velocity.

There are two, or perhaps one may say three, main cases. First, where the friction is great. In that case the recovery is of the nature of a slow leak, according to a decreasing geometrical progression or a logarithmic curve ; the logarithmic decrement being independent of the inertia, and being equal to the quotient of the elasticity and the resistance coefficients.

As the resistance is made less, the recovery becomes quicker and quicker until inertia begins to prominently assert its effect and to once more lengthen out the time of final recovery by carrying the recoiling matter beyond its natural position, and so prolonging the disturbance by oscillations. The quickest recovery possible is obtained just before these oscillations begin ; and it can be shown that this is when the resistance coefficient is equal to twice the geometric mean of the elasticity and the inertia. One may consider this to be the second main case.

The third principal case is when the resistance is quite small, and when the recovery is therefore distinctly oscillatory. If the viscosity were really zero, the motion would be simply harmonic for ever, unless some other mode of dissipating energy were provided ; but if some such mode were provided, or if the viscosity had a finite value, then the vibrations would be simply harmonic with a dying out amplitude, the extremities of all the swings lying on a logarithmic curve. In such a case as this, the rate of swing is practically independent of friction ; it depends only on elasticity and inertia ; and, as is well known for simple harmonic motion, the time of a complete swing is 2π times the square root of the ratio of inertia and elasticity coefficients.

Making the statement more electrically concrete, we may consider a circuit with a certain amount of stored-up potential energy or electrical strain in it : for instance, a charged Leyden jar provided with a nearly complete discharge circuit. The main elastic coefficient here is the reciprocal of the capacity of the jar : the more capacious the jar the more "pliable" it is—the less force of recoil for a given displacement,—so that capacity is the inverse of rigidity. The main inertia coefficient is that which is known electrically as the "self-induction" of the circuit : it involves the inertia of all the displaced matter and ether, of everything which will be moved or disturbed when the jar is discharged. It is not a very simple thing to calculate its value in any given case ; still it can be done, and the general idea is plain enough without under-

standing the exact function and importance of every portion of the surrounding space.

Corresponding, then, to the well-known simple harmonic $T = 2\pi \sqrt{\frac{m}{k}}$, we have, writing L for the self-induction or inertia of the circuit, and S for its capacity or inverse rigidity constant,

$$T = 2\pi \sqrt{LS},$$

This, therefore, is the time of a complete swing. Directly the jar is discharged, these oscillations begin, and they continue like the vibration of a tuning-fork until they are damped out of existence by viscosity and other modes of dissipation of energy.

But now just consider a tuning-fork. Suppose its substance were absolutely unviscous, would it go on vibrating for ever? In a vacuum it might: in air it certainly would not. And why not? Because it is surrounded by a medium capable of taking up vibrations and of propagating them outwards without limit. The existence of a vibrating body in a suitable medium means the carving of that medium into a succession of waves and the transmission of these waves away into space or into absorbing obstacles. It means, therefore, the conveyance away of the energy of the vibrating body, and its subsequent appearance in some other form wherever the radiating waves are quenched.

The laws of this kind of wave-propagation are well known; the rate at which waves travel through the medium depends not at all on any properties of the original vibrating body, the source of the disturbance; it depends solely on the properties of the medium. They travel at a rate precisely equal to the square root of the ratio of its elasticity to its density.

Although the speed of travel is thus fixed independently of the source, the length of the individual waves is not so independent. The length of the waves depends both on the rate at which they travel and on the rate at which the source vibrates. It is well known and immediately obvious that the length of each wave is simply equal to the product of the speed of travel into the time of one vibration.

But not every medium is able to convey every kind of vibration. It may be that the mode of vibration of a body is entirely other than that which the medium surrounding it can convey: in that case no dissipation of energy by wave-propagation can result, no radiation will be excited. The only kind of radiation which common fluids are mechanically able to transmit is well known: it is that which appeals to our ears as sound. The elasticity concerned in such disturbance as this is mere volume elasticity or incompressibility. But electrical experiments (the Cavendish experiment,¹ and Faraday's ice-pail experiment) prove the ether to be enormously—perhaps absolutely—incompressible; and if so, such vibrations as these would travel with infinite speed and not carve proper waves at all.

Conceivably (I should like to say probably) *gravitation* is transmitted by such longitudinal impulses or thrusts, and in that case it is nearly or quite instantaneous; and the rate at which it travels, if finite, can be determined by a still more accurate repetition of the Cavendish experiment than has yet been made; but true radiation transmitted by the ether cannot be of this longitudinal character. The elasticity possessed by the ether is of the nature of rigidity: it has to do with shears and distortions; not mechanical stresses, indeed—to them it is quite limpid and resistless—but electromotive stresses: it has an electrical rigidity, and it is this which must be used in the transmission of wave-motion.

But the oscillatory discharge of a Leyden jar is precisely competent to apply to the ether these electromotive vibrations: it will shake it in the mode suitable for it to

transmit; and accordingly, from a discharging circuit, waves of electrical distortion, or transverse waves, will spread in all directions at a pace depending on the properties of the medium.

Thus, then, even with a circuit of perfect conductivity the continuance of the discharge would be limited, the energy would be dissipated; not by friction, indeed—there would in such a circuit be no direct production of heat—it would be dissipated by radiation, dissipated in the same way as a hot body cooling, in the same way as a vibrating tuning-fork mounted on its resonant box. The energy of the vibrating body would be transferred gradually to the medium, and would by this be conveyed out and away, its final destination being a separate question, and depending on the nature and position of the material obstacles it meets with.

Velocity of Electrical Radiation.

The pace at which these radiation-waves travel depends, as we have said, solely on the properties of the medium, solely on the relation between its elasticity and its density. The elasticity considered must be of the kind concerned in the vibrations; but the vibrations are in this case electrical, and so electrical elasticity is the pertinent kind. This kind of elasticity is the only one the ether possesses of finite value, and its value can be measured by electrostatic experiments. Not absolutely, unfortunately: only the relative elasticity of the ether as modified by the proximity of gross substances has yet been measured: its reciprocal being called their specific inductive capacity, or dielectric constant, K . The absolute value of the quantity K is at present unknown, and so a convention has arisen whereby in air it is called 1. This convention is the basis of the artificial electrostatic system of units. No one supposes, or at least no one has a right to suppose, that its value is really 1. The only rational guess

at its value is one by Sir William Thomson,¹ viz. $\frac{1}{842.8}$. Whether known or not, the absolute value of the dielectric constant is manifestly a legitimate problem which may any year be solved.

The other thing on which the speed of radiation waves depends is the medium's density—its electric density, if so it must be distinguished. Here, again, we do not know its absolute value. Its relative or apparent amount inside different substances is measured by magnetic experiments, and called their specific magnetic capacity, or permeability, and is denoted by μ .

Being unknown, another convention has arisen, quite incompatible with the other convention just mentioned, that its value in air shall be called 1. This convention is the basis of the artificial electro-magnetic system of units—volts, ohms, amperes, farads, and the like. Both of these conventions cannot be true: no one has the least right to suppose either true. The only rational guess at ethereal free density is one by Sir William Thomson, viz. 9.36×10^{-10} .

Very well, then; it being clearly understood that these two great ethereal constants, k or $\frac{1}{K}$, and μ , are neither of them at present known, but are both of them quite knowable, and may at any time become known, it remains to express the speed of wave transmission in terms of them. But it is well known that this speed is simply the square root of the ratio of elasticity to density, or

$$v = \sqrt{\frac{k}{\mu}}, \text{ or } \frac{1}{\sqrt{K\mu}}.$$

This then is the speed with which waves leave the discharging Leyden jar circuit, or any other circuit conveying alternating or varying currents, and travel out into space.

Not knowing either k or μ , we cannot calculate this

¹ See Maxwell's "Electrical Researches of Cavendish," p. 104; see also p. 417.

¹ Trans. R. S. Edin., xxi. 60; see also article "Ether," in the "Encyc. Brit."

speed directly, but we can try to observe it experimentally.

The first and crudest way of making the attempt would be to arrange a secondary circuit near our oscillating primary circuit, and see how soon the disturbance reached it. For instance, we might take a nearly closed loop, make it face a Leyden jar circuit across a measured distance, and then look for any interval of time between the spark of the primary discharge and the induced spark of the secondary circuit, using a revolving mirror or what we please. But in this way we should hardly be able to detect any time at all: the propagation is too quick.

We might next make use of the principle of the electric telegraph, viz. the propagation of a disturbance round a single circuit from any one point of origin. Consider a large closed circuit, either conveying or not conveying a current: introduce at any one point a sudden change—a sudden E.M.F., for instance, or a sudden resistance if there be a current already. Out from that point a disturbance will spread into the ether, just as happens in air when a blow is struck or gun-cotton fired. A regular succession of disturbances would carve the ether into waves: a single disturbance will merely cause a pulse or shock; but the rate of transmission is the same in either case, and we may watch for the reception of the pulse at a distant station. If the station has to be very distant in order to give an appreciable lapse of time, a speaking-tube is desirable to prevent spreading out in all directions—to concentrate the disturbance at the desired spot. What a speaking-tube is to sound, that is the wire of the circuit—the telegraph wire—to ethereal pulses.

It is a curious function, this of the telegraph wire: it does not *convey* the pulses, it directs them. They are conveyed wholly by the ether, at a pace determined by the properties of the ether, modified as it may be by the neighbourhood of gross matter. Any disturbance which enters the wires is rapidly dissipated into heat, and gets no further; it is the insulating medium round it which transmits the pulses to the distant station.

All this was mentioned in Part III., and an attempt was made to explain the mechanism of the process, and to illustrate in an analogical way what is going on.

The point of the matter is that currents are not propelled by end-thrusts, like water in a pipe or air in a speaking-tube, but by lateral propulsion, as by a series of rotating wheels with their axes all at right angles to the wire surrounding it as a central core, and slipping with more or less friction at its surface. This is characteristic of ether modes in general: it does not convey longitudinal waves or end-thrust pulses, like sound, but it conveys transverse vibrations or lateral pulses, like light.

Without recapitulating further, we can perceive, then, that the transmission of the pulse round the circuit to its most distant parts depends mainly on the medium surrounding it. The process is somewhat as follows:—Consider two long straight parallel wires, freely suspended, and at some great distance joined together. At the near end of each, start equal opposite electromotive impulses, as by suddenly applying to them the poles of a battery; or apply a succession of such pulses by means of an alternating machine. Out spread the pulses into space, starting in opposite phases from the two wires, so that at a distance from the wires the opposite pulses interfere with each other, and are practically non-existent, just as but little sound is audible at a distance from the two prongs of a freely suspended tuning-fork. But near the wires, and especially between them, the disturbance may be considerable. To each wire it spreads and is dissipated, and so a fresh supply of energy goes on continually arriving at the wires, always flowing in from outside, to make up the deficiency. If the wires are long enough hardly any energy may remain by the time their distant ends are reached; but whatever there is will still be crowding in upon the wires and getting dissipated, unless by

some mechanism it be diverted and utilized to effect some visible or audible or chemical change, and so to give the desired signal.

Now the pace at which this transmission of energy goes on in the direction of the wires is pretty much the same as in free space. There are various circumstances which can retard it; there are none which can accelerate it. The circumstances which can retard it are, first, constriction of the medium by too great proximity of the two conducting wires: as, for instance, if they consisted of two flat ribbons close together with a mere film of dielectric between, or if one were a small-bore tube and the other its central axis or core. In such cases as this the general body of ether takes no part in the process, the energy has all to be transmitted by the constricted portion of dielectric, and the free propagation of ethereal pulses is interfered with: the propagation is no longer a true wave-propagation at all, but approximates more or less closely to a mere diffusion creep, rapid it may be, and yet without definite velocity, like the conduction of heat or the diffusion of a salt into water. One well-known effect of this is to merge successive disturbances into one another, so that their individuality, and consequently the distinctness of signalling, is lost.

Another circumstance which can modify rate of transmission of the pulses is ethereal inertia in the substance of the conducting wires, especially extra great inertia, as, for instance, if they are made of iron. For the dissipation of energy does not go on accurately at their outer surface; it has usually to penetrate to a certain depth, and until it is dissipated the fresh influx of energy from behind does not fully occur. Now, so long as the value of μ for the substance of the wires is the same as that of air or free space, no important retardation is thus caused, unless the wires are very thick; but directly the inertia in the substance of the wires is one or two hundred times as big as that outside, it stands to reason that more time is required to get up the needful magnetic spin in its outer layers, and so the propagation of pulses is more or less retarded. At the same time this circumstance does not alter the character of the propagation, it does not change it from true wave velocity to a diffusion, it leaves its character unaltered; and so the signals, though longer in coming, may arrive quite clear, independent, and distinct. It is much the same, indeed, as if the density of the surrounding medium had been slightly increased.

These, then, are the main circumstances which affect the rate of transmission of a pulse from one part of a closed circuit to another: extra inertia or so-called magnetic susceptibility in the conducting substance, especially in its outer layers; and undue constriction or throttling of the medium through which the disturbance really has to go. Both these circumstances diminish rate of transmission, and one (the last mentioned) modifies the law and tends to obliterate individual features and to destroy distinctness.

Of course, besides these, the nature of the insulating medium will have an effect on the rate of propagation, but that is obvious all along; it is precisely the rate at which any given medium transmits pulses that we want to know, and on which we are thinking of making experiments. If we use gutta-percha (more accurately the ether inside gutta-percha) as our transmitting medium in an experiment, we are not to go and pretend that we have obtained a result for air.

The circumstances we have considered as modifying the rate of transmission are both of them adventitious circumstances, independent of the nature of the medium, and they are entirely at our own disposal. If we like to throttle our medium, or to use thick iron wires, we can do so, but there is no compulsion: and if we wish to make the experiment in the simplest manner, we shall do no such thing. We shall use thin copper wires (the thinner the better), arranged parallel to one another a fair distance

apart, and we shall then observe the time which an electromotive impulse communicated at one end takes to travel to the other. Instead of using two wires, we may if we like use what comes to much the same thing, viz. a single wire suspended at a reasonable height above the ground, as in a common land telegraph. Such a case as this is much the same as if two wires were used at a distance apart equal to about twice the height above the ground.

The experiment, if it could be accurately made, would result in the observation of a speed of propagation equal to 3×10^{10} centimetres per second. The actual speed in practice may be less than this, by reason of the various circumstances mentioned, but it can never be greater. This, then, is the rate of transmission of transverse impulses, and therefore of transverse waves, through ether as free as it can be easily obtained.

There are many methods known to physicists by which an indirect experimental determination of this velocity can be made. These methods are more easily practicable than the one described: they directly determine the ratio k/μ , or, what is the same thing, the product $K\mu$, and it is left to theory to say that this is really the velocity of electrical pulses in free ether. It is unnecessary to say more about them here.

OLIVER J. LODGE.

(To be continued.)

A HISTORY OF THE AUGUST METEORS.

THE August meteor-shower has been more frequently observed than any other with which we are acquainted, and the modern history of this remarkable system includes many interesting circumstances. It has not, in recent times, given us displays equal in grandeur to periodical swarms like the Leonids of November 13 and Andromedes of November 27, being decidedly less rich in point of numbers. But what this stream lacks in this respect is compensated for by the annual visibility of the shower and by the intense brilliancy of some of its individual members. Every year the August meteors present a conspicuous appearance on the night following St. Lawrence's Day, and fire-balls of excessive lustre are now and then interspersed with the smallest perceptible shooting-stars of the system. The Leonids and Andromedes, which have rendered the month of November so famous in meteoric annals, can only reappear abundantly at intervals of thirty-three and (probably) thirteen years, whereas the Perseids of August are unfailing in their regular apparitions as the epoch comes round each year. On the night of the 10th the most casual observer will not fail to notice the surprising frequency of shooting-stars, and must remark their occasional brilliancy and the persistency of the phosphorescent after-glows which they generate during their rapid flights amongst the fixed stars.

The early history of the August meteors is vague and meagre in the extreme. Ancient writings are significantly mute as to the scientific aspect of meteor-showers. Doubtless in olden times these phenomena were equally as plentiful as at present, but amid the ignorance and superstition which prevailed they were little regarded. The prominent part which meteors play in the solar system was not suspected, hence no importance was attached to their appearance. They were supposed to be mere exhalations uncontrolled by fixed laws, and it is entirely due to modern science that their true character has been revealed, and that they have been raised to the dignity of bodies having a celestial origin, and probably also an extensive influence throughout the wide range of astronomical physics.

But former records, if void of particulars possessing a scientific utility, are yet often useful in supplying dates. Many old references to meteor-showers, though very imperfect in description, are, by the accordance of epoch, justly assumed to have been early exhibitions of the very

same systems as those which have furnished some of the most imposing displays of recent years. In the catalogue of 315 meteoric showers compiled by Quetelet, a considerable proportion are probably identical with the August Perseids, and below we give the dates, up to a century ago, of these:—

Year.	Date.	Year.	Date.
811	July 25	926	July 27-30
820	" 25-30	933	" 25-30
824	" 26-28	1243	Aug. 2
830	" 26	1451	" 7
833	" 27	1709	" 8
835	" 26	1779	" 9 and 10
841	" 25-30	1781	" 8
924	" 27-30	1784	" 6
925	" 27-30	1789	" 10

The dates in the ninth and tenth centuries are somewhat different from those in later years, but this does not negative the assumed relation, because they are brought nearly into agreement when the change of style in 1752 is allowed for. This proves the showers to have really occurred at a period early in August according to present reckoning. There may also be a slight alteration in the epoch of the swarm due to a shifting of the node, which, in its cumulative amount after many ages, might reach a considerable value. For the reasons assigned, the celebrated shower of Leonids which now takes place on November 13 was observed in October 902, and again on October 19, 1202, October 22, 1366, &c.

Muschenbroek, in 1762, announced the general fact that he had observed shooting-stars to be more plentiful in August than in any other month of the year. Further towards the close of the century this was in part confirmed by the apparition of many meteors on August 8 and 9. In 1806 and 1812, Dr. Forster, of Clapton, recorded in his "Calendar" that these phenomena were unusually abundant on August 10, and in the latter year he particularly noted the extraordinary length and phosphorescent aspect of the trains left in their wake. Subsequently the same epoch was amply corroborated; and in 1835, Quetelet definitely mentioned the 9th and 10th of August as the date of maximum annual display.

On August 9, 1837, M. Wartman, of Geneva, observed 82 of these meteors between 9 p.m. and midnight. In the following year, on August 10, observations were made at Geneva and at Planchettes, a village 62 miles north-east of Geneva, with the view of determining the heights and velocities of the meteors. A discussion of the results showed that the average elevation above the ground was 550 miles, and the velocity 220 miles, but these figures are now known to have been enormously in excess of the true values.

From 20 meteors observed in August 1863, Prof. A. S. Herschel determined the mean height as 81.6 miles at first appearance and 57.7 miles at disappearance, and the velocity was found to be 34.4 miles per second. From 27 meteors similarly observed in Italy between August 5 and 10, 1864, Secchi derived limiting heights of 76.6 and 49.7 miles; and, averaging these with the results obtained by Prof. Herschel in the preceding year, we get 78 to 54 miles, which may be adopted as representative values for the normal heights not only of the Perseids, but of shooting-stars generally.

Heis, Schmidt, Greg, and Herschel were amongst the first to methodically observe the August meteor-shower and determine its radiant point in the northern region of Perseus. In 1863, August 10, an unusual display was witnessed, for on this occasion the stream seems to have attained a degree of intensity not recorded either before or subsequently to that year. In 1871 there was also a very pronounced and abundant appearance of these meteors. In NATURE, vol. xx. p. 457 (September 11, 1879), will be found some details as to the relative number of August meteors counted in different years.